

Summary: Comments on the State of our Subject

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Abstract. This conference shows the impressive rate of advances in the observations and theoretical interpretations of large-scale structure. But to explain my feeling that we may still have a lot to learn I offer some comments on our sociology, informed by social construction, in adopting lines of thought that tend not to follow straightforward readings of what is observed. Thus galaxies are thought to be affected by environment, not only in the morphology-density relation, but also as the cause of biased formation. But the Tully-Fisher and fundamental plane relations do not allow much room for the contingencies of environment. It is thought that the galaxy distribution is not likely to be closely related to the mass, yet most dynamical analyses indicate that if the mean density is low then optically selected galaxies are good mass tracers. The evidence is that at redshift unity the larger spiral and elliptical galaxies were by and large in place, as were a good fraction of the great clusters of galaxies. These galaxies evolve slowly at $z < 1$, yet we hear of rapid evolution not much before this, in galaxy formation. Perhaps all these examples are misleading. But, as Kuhn has taught us, complex interpretations of simple phenomena have been known to be precursors of paradigm shifts.

1. Cautionary Remarks

The experimental proof of phlogiston seemed incontrovertible Indispensable as it was as a chemical concept, phlogiston as a thing was elusive; it was widely believed to be the “least accurately known” of all chemical substances or principles, incapable of being isolated and studied on its own. In Cavendish the Experimental Life, Jungnickel & McCormach (1999, p. 201)

We will remember this as a golden age for our subject, now strongly driven by observations as well as theory. But lest we be remembered for incautiously harboring another phlogiston I feel some cautionary remarks are in order.

I begin with a lesson to be drawn from the cluster MS1054 (at redshift $z = 0.83$). The remarkably detailed observations of this cluster are featured in several of the talks at this conference. The velocity dispersion of the galaxies, from the spread of redshifts of their spectra, and the temperature of the intracluster plasma, from the X-ray spectrum, yield consistent measures of a gravitational potential well deep enough that standard physics says it is capable of lensing more distant galaxies, and the effect is seen (Tran et al. 1999). That is, we have

excellent reason to believe this is a massive cluster of galaxies, characterized in some detail, and quite similar to nearby ones. The familiar and sensible reaction is that a viable theory of structure formation must predict the existence of a significant number of rich clusters at redshift $z \sim 1$. But let us pause to consider another lesson: this is a vivid illustration of the success of textbook physics.

The analysis of the observations of MS1054 extrapolates standard physics from the length scales on which it was discovered and tested — the Solar System and smaller — to an object some ten orders of magnitude larger than the Solar System. It is based on the detection of electromagnetic radiation at X-ray, optical and (for other objects at similar distances) radio wavelengths that has been propagating toward us for half the time our universe has been expanding, covering a distance comparable to the relativistic particle horizon (the limiting distance allowed by causality in the absence of inflation or whatever finesses the relativistic singular beginning of a universe with positive pressure). We are presented with a well cross-checked picture of a cluster operating according to the physics that was in our textbooks well before technology permitted its application on such enormous scales of size and distance. One can adduce many more examples of the success of standard physics, of course. Physical scientists may seem to our colleagues in sociology a little didactic if not downright arrogant in our advertisement of textbook physics as a good approximation to an objective physical reality, but this is a response to exceedingly strong reinforcement.

We have to be a little more cautious about the lines of research we have chosen to follow in seeking to add to textbook physics; the choice has to be at least in part a social decision. Hacking (1999) discusses “sticking points” that tend to hang up debates on physical science as a social construction as opposed to an approach to an external objective reality.¹ One point is contingency: if we can see research paths not taken we have to credit the path the community chose with an element of social construction. Another is stability: if the chosen path leads to unexpected and successful predictions we are encouraged to think it has led to an approximation to a physical reality. On both points nonbaryonic dark matter is a good example of a social construction.

We did not have to adopt the hypothesis of nonbaryonic dark matter; our community could have sought to extend Milgrom’s modification of Newtonian dynamics (Brada & Milgrom 1999 and references therein) to a modified relativistic cosmology without dark matter. Given the full attention and energy of our best and brightest, and a few free parameters, maybe we could have come up with a reasonable fit to a reasonable number of the observations. It was sensible that most of us chose to stay with textbook gravity physics unless or until we’re forced away from it, leaving a few of our company to explore the options. But this was a community choice of the kind described in social construction.

The dark matter hypothesis has been stable — apart from the shift to a nonbaryonic form — since its introduction more than half a century ago (Zwicky 1933), which is encouraging. Since Zwicky we have learned that (within standard physics) the more isolated spiral galaxies have massive dark halos that are well correlated with the optical luminosity, that dark matter is the dominant mass in

¹All I know about social construction is what I read in the reviews. I have been most influenced by Hacking’s (1999) “foreign correspondent” report.

groups and clusters of galaxies, and that it's hard to find dark mass concentrations unaccompanied by optical objects. Nonbaryonic dark matter plays an important role in the most widely discussed and successful model for structure formation, the adiabatic cold dark matter (Λ CDM) model, but the tests of this model are not yet all that tight, and we do not know how well we could have done if we had not had this concept. In short, I don't think we can argue that nonbaryonic cold dark matter has proved to be considerably more stable than the phlogistical chemistry that occupied much of Henry Cavendish's scientific career (Jungnickel & McCormach 1999). Lavoisier's new chemistry — without caloric — evolved into the standard model because of its experimental successes, in one of Kuhn's (1962) paradigm shifts. Nonbaryonic dark matter could force its way into standard physics on far less extensive grounds than we have for chemistry, but we do need hard evidence. If the precision measurements of the angular distribution of the thermal cosmic background radiation fit one of the Λ CDM models now under discussion, after adjustment of the model parameters to values that fit the astronomy, it will make believers of most of us. But we're not there yet.

It's a natural tendency in our community to act as if we knew nonbaryonic cold dark matter exists, as real and firmly established as the cluster MS1054. We've seen spectacular runs of successes in physical science, and it's reasonable enough to hope that we have another one, that in Λ CDM we've hit on a good approximation to how structure forms after only a few (apparently) false starts — explosions, hot dark matter, baryonic dark matter, cosmic strings, and textures. This is a positive attitude; it leads most of our community to focus its attention on one line of thought, pushing it until the idea proves useful or breaks. It is a little less benign when talking to people in other sciences, or the media; they can't be expected to factor in our personal equations. And we must take care not to fool ourselves. In this connection I would point to a few clouds on the horizon.

2. Sticking Points

The theme for this list of four sticking points (following Hacking 1999) in the search for a standard model for structure formation is an apparent incongruity between simple phenomena and complicated interpretations. The complications are inspired by the Λ CDM model. If this model had the empirical support of quantum mechanics I would have to agree that these very likely are examples of complex situations that only happen to look simple.² But we are searching the phenomena for critical tests of the Λ CDM model.

The first sticking point is that if the density parameter in matter that is capable of clustering (thus excluding a term in the stress-energy tensor that

²If cold fusion were observed in the laboratory it would mean quantum mechanics has unambiguously failed. Since this would happen on scales where quantum mechanics has been abundantly tested it is an excellent bet that cold fusion fails. Here is a case where it makes sense to pay great attention to what the theory says, while of course bearing in mind the slight chance that we have something to learn about fundamental physics on scales where all evidence so far has been that we already have an excellent approximation to physical reality.

acts like Einstein’s cosmological constant) is low, $\Omega_m = 0.25 \pm 0.15$, as is pretty generally accepted these days, then optically selected galaxies are good tracers of mass. Most dynamical studies are consistent with the assumption that the dark mass is in galaxy halos with roughly flat rotation curves and radii of a few hundred kiloparsecs.³ So why do we hear so much about biasing, about significant differences between the distributions of galaxies and mass? It is in part a response to another observation: spirals, ellipticals, and radio galaxies have different distributions; they cannot all trace the mass. But another reason is that biasing follows very naturally from the Λ CDM model for structure formation.

The preference of early-type galaxies for denser environments is consistent with a biasing picture, and good analogs happen in Λ CDM model numerical simulations. But how do we reconcile the contingency of environmental influences on galaxies with the regularity of the fundamental plane for ellipticals and the spheroid components of spiral galaxies? A straightforward reading would be that the spheroids generally have been only mildly affected by the environmental influences of tides and mergers. Some spheroids acquired thin disks; the Tully-Fisher relation says the resulting luminosity correlates well with the gravity that determines the rotation curve. A simple inference would be that, after the decision to acquire a disk, these galaxies also have evolved as island universes.

The second point is the issue of what if anything is in the voids defined by the large galaxies. Most known objects avoid them. This is true of dwarf and irregular galaxies, and of high surface density gas clouds detected by absorption lines or HI 21 cm emission. Low surface density galaxies also avoid the dense concentrations of high surface density ones, maybe because the large tidal fields disrupt them, but they also respect the voids (Schombert, Pildis, & Eder 1997). Very low surface density gas clouds are strongly excluded from the neighborhood of large galaxies, maybe for similar reasons; they tend to prefer the edges of concentrations of galaxies (Shull, Penton, & Stocke 1999). The straightforward reading is that the voids are pretty close to empty. A robust prediction of the Λ CDM model is that the voids contain a considerable mass fraction. Elegant numerical simulations of galaxy formation within this model indicate galaxies of all types, classified by their star formation histories within the model, respect common voids, in excellent agreement with the observations (eg. Cen & Ostriker 1998, fig. 2). This impressive result must be taken seriously, but I find a full suspension of disbelief difficult. In the model simulations the matter in the voids is in clumps — dark matter potential wells — capable of producing gas clouds or star clusters. Potential wells with escape velocities less than about 20 km s^{-1} cannot gravitationally hold baryonic matter heated by photoionization by the intergalactic ionizing radiation, but there is plenty of room between that and the escape velocity of a large galaxy for the formation of dwarf or irregular void galaxies. We know galaxies with low internal velocities can form — they are present in great abundance in the neighborhood of large galaxies — so why are they so rare in the voids? Could the void matter in the Λ CDM model really

³An exception is in clusters of galaxies, where the dark mass is smoothly distributed — maybe a result of tidal stripping — and M/L is high — a readily understandable effect of environment.

have almost totally failed to produce observable objects that would be expected to bear the stigmata of a troubled youth in a hostile environment?

The third point is another aspect of biasing, in the relation between the low order correlation functions of galaxies and mass. The two-point function for optically selected galaxies is quite close to a power law, $\xi(r) \propto r^{-\gamma}$, $\gamma = 1.77 \pm 0.04$, over nearly three orders of magnitude of separation, from a few tens of kiloparsecs to about 10 Mpc (when scaled to $H_o = 100 \text{ km s}^{-1} \text{ Mpc}^{-1}$). The power law index γ changes little back to $z \sim 1$. The three- and four-point galaxy functions are less accurately measured, but are consistent with a simple scale-free clustering hierarchy on scales less than a few megaparsecs. The aCDM models predict that the ratio of galaxy and mass two-point functions varies with time and separation. The models can be adjusted to make the galaxy function a close approximation to a power law, with index γ that is close to stable back to $z \sim 1$, out of the more complicated behavior of the mass correlation functions (eg. Pearce et al. 1999). This is impressive but curious: since mass controls dynamics why would the galaxies considered as mere froth display the power law regularity?

At this meeting Adelberger presents striking evidence that the galaxy and mass two-point functions are quite different at redshift $z \sim 3$, as predicted by aCDM model simulations. We will have to see whether this is a property of galaxies in general at $z \sim 3$ or an analog of the strong clustering of present-day radio-selected relative to optically selected galaxies. If the former, then the galaxies between the concentrations of Lyman-break systems observed by Adelberger *et al.* have to have formed later than $z = 3$, as predicted by aCDM models. But that leads to the fourth apparent incongruity.

The observations indicate that at redshift $z = 1$ the bulk of the high surface density ellipticals have formed and settled to their fundamental plane, after correction for evolution of the star populations, that much the same is true of the large spiral galaxies, and that there is in addition a population of more rapidly fading lower luminosity galaxies that maybe end up as the abundant present-day low luminosity low velocity dispersion companions of the giants. At this conference we heard that there is not a lot of evidence for evolution of the rich clusters of galaxies: at $z \sim 1$ they tend to be less well relaxed, and maybe they tend to be less massive, but not by a large factor. One gets the impression that an astronomer sent back in time to $z = 1$, with a telescope and all technical support, would recognize the objects that will evolve, as island universes or close to it, into what appear in our textbooks of astronomy. Continuity might suggest the evolution of structure was not dramatically different in the last factor of two expansion, from $z = 1$ to the present, and in the previous factor of two, $z = 3$ to $z = 1$.

There is some evidence for this. Prochaska & Wolfe (1998) make a case for the interpretation of the damped Lyman α absorbers (DLAs) as young disk galaxies, already assembled at $z \sim 3$. Gilmore (1999) and colleagues find evidence for small scatter in ages of the stars in the old spheroid population of our galaxy, and indications of a similar situation in other large galaxies. If the stars formed at $z \sim 1$ it would require a most curious synchronization; it's easier to think the stars formed early, when the age of the expanding universe was comparable to the spread in star ages in the first large generation. The stars could

have formed early but assembled in galaxies much later, but this late assembly would require a large collapse factor.⁴ If there were large collapse factors in the gravitational assembly of galaxies at $z \sim 1$, why don't we see large collapse factors at lower redshift?⁵ There is clear evidence of merging and accretion at $z \lesssim 1$, but as a perturbation to the big picture of the evolution of galaxies and the central parts of clusters of galaxies as nearly isolated and stable island universes. If galaxies were assembled early, at $z \gg 1$ it would require more modest and maybe more reasonable-looking collapse factors.

As mentioned above, the Lyman break objects do present evidence for late galaxy formation, and this is in line with the observed rapid decline in the mean star formation rate from $z = 1$ to the present (Madau 1999; Steidel et al. 1998). But the mean star formation rate is close to flat at $1 \lesssim z \lesssim 3$, and the density parameter in hydrogen in DLAs shows a monotonic increase with increasing redshift, reaching a value comparable to that now in stars in spirals at $z = 3$ (Storrie-Lombardi & Wolfe 1999), as if the baryons had already been assembled in protogalaxies by $z = 3$.

The Λ CDM model prediction of late galaxy formation, peaking at $z \sim 1$, is not inconsistent with the evidence that spirals and ellipticals are present at $z \sim 1$, and it certainly is not inconsistent with the more confused observational picture of what happened earlier. But it is curious that the proposed rapid evolution of structure at $1 \lesssim z \lesssim 3$ is so different from the mild gravitational evolution seen in the last factor of two expansion.

These four points show aspects of present-day research in structure formation that Kuhn (1992) might characterize as typical of the precursor of a paradigm shift. Maybe this is a result of the intense search for empirical regularities as clues to how galaxies formed; maybe some of our sticking points are social constructions rather than true phenomena. Maybe others are examples of the simple patterns that can come out of complex physics. And maybe some indicate we still have things to learn about the physical basis for the evolution of structure at $z \lesssim 3$.

3. The State of Cherished Hypotheses

Most of us would agree that there are no pressing crises in cosmology, in the sense that the paradigms have been easily adjusted to consistency with the observational advances. There are clouds on the horizon, that may be only glitches or may signal changes to come. To organize our thoughts let us consider how we might react to failures of common hypotheses.

⁴At the line of sight velocity dispersion characteristic of an L_* elliptical, $\sigma = 150 \text{ km s}^{-1}$, and at redshift $z = 1$, the mean density within the radius $10h^{-1} \text{ kpc}$ is about 3×10^4 times the background value.

⁵An example would be the collapse of the Local Group, in a crossing time, all the way to the merging of the Milky Way and M31. That would require that the relative motion is close to radial. But numerical solutions indicate the transverse velocity is comparable to the radial velocity of approach of these two galaxies (Peebles 1994), a result of the perturbation by neighboring mass concentrations. If this is so the Local Group is not going to collapse much further. And neither are the great clusters, according to the evidence we heard at this meeting.

Nonbaryonic dark matter is hypothetical but well motivated: it reconciles the dynamical measures of the mean mass density with the baryon density in the standard model for the origin of deuterium, and it has simplified the search for a model for structure formation. Most of us would be quite surprised to lose nonbaryonic dark matter, though we certainly would be comforted to see it acquire some color, as an interaction in the laboratory.

The consensus is that the mass density in matter capable of clustering is $\Omega_m = 0.25 \pm 0.15$. The case is not definitive, and it implies we flourish at a special epoch, but I think few of us expect to see a return to the Einstein-de Sitter model. The case for a mass component that acts like Einstein's cosmological constant depends on the SNeIa redshift-magnitude relation and the spectrum of angular fluctuations of the thermal cosmic background radiation. The former is a beautiful but difficult measurement. The latter is work in progress. A shift to an open model might not be traumatic.

Gravitationally driven hierarchical structure formation is well observed, in the collapse of the Local Group, virgocentric flow, merging of clusters, and merging of individual galaxies. This might be counted as part of the established standard model for structure formation (with an assist from nongravitational processes, as in intracluster plasma and galactic winds). It is sensible to extrapolate to the hypothesis of hierarchical assembly of galaxies. Nearby galaxy merging events offer a reasonable model for what is seen at high redshift, and this has led to the conclusion that galaxy merging was a lot more common at $z \sim 1$: perhaps we are seeing the last stages of hierarchical assembly of the galaxies. But irregular morphology need not signify youth; the assembly of the mass concentration in a galaxy, whether by hierarchical growth or monolithic collapse, could happen well before the distribution of light has relaxed to a textbook galaxy. The many close pairs of galaxies in the cluster MS1054 could signal merger events about to happen, or maybe they indicate the cluster has recently accreted some loose groups that are falling apart. Though I would be startled to see the failure of the paradigm of gravitational growth of clustering on relatively large scales, I see a less strong case on comoving scales $\lesssim 100$ kpc.

The evidence is that at $z = 1$ the large galaxies and the great clusters of galaxies are not greatly different from today, and present in comparable numbers. There is a considerably higher global rate of star formation, maybe because many galaxies have just formed, or maybe because it took a long time to convert gas to stars in systems that had been assembled as mass concentrations much earlier. This meeting featured discussions of an elegant possible signal of galaxy formation, an increase of the comoving galaxy clustering length at redshifts approaching the epoch of first generation of galaxies. There is observational evidence of this effect, at $z \sim 3$, as predicted by the Λ CDM model for structure formation. But the last section lists reasons why some of us might not be entirely surprised to see some fundamental adjustments of the picture.

We have strong faith in the stability of textbook physics, for good reason. It is not unnatural to have pretty strong faith in our ideas on how to add to the textbook physics, as the Λ CDM model, but sensible to be a little cautious because we are attempting to draw large conclusions from limited evidence. At the rate people are improving the observations, and our understanding of the predictions of the Λ CDM model, as documented in these Proceedings, we may

soon learn whether we have already have a theory for galaxy formation that is stable enough to merit promotion to the textbooks, or whether we are going to have to cast our nets for hypotheses a little more broadly. Each of us has an opinion on which outcome would be the more surprising.

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References

- Brada, R. & Milgrom, M. 1999, ApJ, 519, 590
 Cen, R. & Ostriker, J. P. 1998, astro-ph/9809370
 Gilmore, G. 1999, Nature, 400, 402
 Hacking, I. 1999, *The Social Construction of What?* (Harvard: Harvard University Press)
 Jungnickel, C. & McCormach, R. 1999, *Cavendish the Experimental Life* (Cranbury N. J.: Bucknell University Press, the Associated University Presses)
 Kuhn, T. S. 1962, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press)
 Madau, P. 1999, in *Particle Physics and the Universe* (Enkoping, Sweden, August 1998), astro-ph/9902228
 Pearce, F. R. et al. 1999, ApJ, 521, L99
 Peebles, P. J. E. 1994, ApJ, 429, 43
 Prochaska, J. X. & Wolfe, A. M 1998, ApJ, 507, 113
 Schombert, J. M., Pildis, R. A., & Eder, J. A. 1997, ApJ Suppl, 111, 233
 Shull, J. M., Penton, S. V., & Stocke, J. T. 1999, astro-ph/9901280
 Steidel, C. C. et al. 1998, astro-ph/9812167
 Storrie-Lombardi, L. & Wolfe, A. M. 1999, private communication
 Tran, K.-V. H. et al. 1999, astro-ph/9902349
 Zwicky, F. 1933, Helv Phys Acta, 6, 110